

SCIENCE

FRIDAY, SEPTEMBER 23, 1910

CONTENTS

<i>Address to the Mathematical and Physical Section of the British Association for the Advancement of Science: PROFESSOR E. W. HOBSON</i>	385
<i>Grants by the British Association</i>	403
<i>Scientific Notes and News</i>	404
<i>University and Educational News</i>	406
<i>Discussion and Correspondence:—</i>	
<i>The Spectrum of Mars: G. R. AGASSIZ, DR. W. W. CAMPBELL</i>	407
<i>Quotations:—</i>	
<i>The British Association</i>	408
<i>Scientific Books:—</i>	
<i>Osborne on The Vegetable Proteins: PROFESSOR LAFAYETTE B. MENDEL. Our Search for a Wilderness: LOUIS AGASSIZ FUETTES</i>	409
<i>Scientific Journals and Articles</i>	411
<i>Special Articles:—</i>	
<i>The Prevention of the Toxic Action of Various Agencies upon the Fertilized Egg through the Suppression of Oxidation in the Cell: PROFESSOR JACQUES LOEB. Oscillations in Electric Discharge: PROFESSOR FRANCIS E. NIPHER</i>	411
<i>The International Geological Congress at Stockholm: PROFESSOR WM. HERBERT HOBBS</i>	413

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

ADDRESS TO THE MATHEMATICAL AND PHYSICAL SECTION OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE¹

SINCE the last meeting of our association one of the most illustrious of the British workers in science during the nineteenth century has been removed from us by the death of Sir William Huggins. In the middle of the last century Sir William Huggins commenced that pioneer work of examination of the spectra of the stars which has insured for him enduring fame in connection with the foundation of the science of astrophysics. The exigencies of his work of analysis of the stellar spectra led him to undertake a minute examination of the spectra of the elements with a view to the determination of as many lines as possible. To the spectroscope he later added the photographic film as an instrument of research in his studies of the heavenly bodies. In 1864 Sir William Huggins made the important observation that many of the nebulae have spectra which consist of bright lines; and two years later he observed, in the case of a new star, both bright and dark lines in the same spectrum. In 1868 his penetrating and alert mind made him the first to perceive that the Doppler principle could be applied to the determination of the velocities of stars in the line of sight, and he at once set about the application of the method. His life-work, in a domain of absorbing interest, was rewarded by a rich harvest of discovery, obtained as the result of most patient and minute investigations. The "Atlas of Representative Stellar Spectra," published

¹ Sheffield, 1910.

in the names of himself and Lady Huggins, remains as a monumental record of their joint labors.

The names of the great departments of science, mathematics, physics, astronomy, meteorology, which are associated with Section A, are a sufficient indication of the vast range of investigation which comes under the purview of our section. An opinion has been strongly expressed in some quarters that the time has come for the erection of a separate section for astronomy and meteorology, in order that fuller opportunities may be afforded than hitherto for the discussion of matters of special interest to those devoted to these departments of science. I do not share this view. I believe that, whilst the customary division into sub-sections gives reasonable facilities for the treatment of questions interesting solely to specialists in the various branches with which our section is concerned, a policy of disruption would be injurious to the wider interests of science. The close association of the older astronomy with mathematics, and of the newer astronomy with physics, forms strong presumptions against the change that has been suggested. Meteorology, so far as it goes beyond the purely empirical region, is, and must always remain, a branch of physics. No doubt, the more technical problems which arise in connection with these subjects, though of great importance to specialists, are often of little or no interest to workers in cognate departments. It appears to me, however, that it is unwise, in view of the general objects of the British Association, to give too much prominence in the meetings to the more technical aspects of the various departments of science. Ample opportunities for the full discussion of all the detailed problems, the solution of which forms a great and necessary part of the work of those who are ad-

vancing science in its various branches, are afforded by the special societies which make those branches their exclusive concern. The British Association will, in my view, be performing its functions most efficiently if it gives much prominence to those aspects of each branch of science which are of interest to a public at least in some degree larger than the circle of specialists concerned with the particular branch. To afford an opportunity to workers in any one department of obtaining some knowledge of what is going on in other departments, to stimulate by means of personal intercourse with workers on other lines the sense of solidarity of men of science, to do something to counteract that tendency to narrowness of view which is a danger arising from increasing specialization, are functions the due performance of which may do much to further that supreme object, the advancement of science, for which the British Association exists.

I propose to address to you a few remarks, necessarily fragmentary and incomplete, upon the scope and tendencies of modern mathematics. Not to transgress against the canon I have laid down, I shall endeavor to make my treatment of the subject as little technical as possible.

Probably no other department of knowledge plays a larger part outside its own narrower domain than mathematics. Some of its more elementary conceptions and methods have become part of the common heritage of our civilization, interwoven in the every-day life of the people. Perhaps the greatest labor-saving invention that the world has seen belongs to the formal side of mathematics; I allude to our system of numerical notation. This system, which, when scrutinized, affords the simplest illustration of the importance of mathematical form, has become so much an indispensable part of our mental furniture that

some effort is required to realize that an apparently so obvious idea embodies a great invention; one to which the Greeks, with their unsurpassed capacity for abstract thinking, never attained. An attempt to do a multiplication sum in Roman numerals is perhaps the readiest road to an appreciation of the advantages of this great invention. In a large group of sciences, the formal element, the common language, so to speak, is supplied by mathematics; the range of the application of mathematical methods and symbolism is ever increasing. Without taking too literally the celebrated dictum of the great philosopher Kant, that the amount of real science to be found in any special subject is the amount of mathematics contained therein, it must be admitted that each branch of science which is concerned with natural phenomena, when it has reached a certain stage of development, becomes accessible to, and has need of, mathematical methods and language; this stage has, for example, been reached in our time by parts of the science of chemistry. Even biology and economics have begun to require mathematical methods, at least on their statistical side. As a science emerges from the stages in which it consists solely of more or less systematized descriptions of the phenomena with which it is concerned in their more superficial aspect; when the intensive magnitudes discerned in the phenomena become representable as extensive magnitudes—then is the beginning of the application of mathematical modes of thought; at a still later stage, when the phenomena become accessible to dynamical treatment, mathematics is applicable to the subject to a still greater extent.

Mathematics shares with the closely allied subject of astronomy the honor of being the oldest of the sciences. When we consider that it embodies, in an abstract

form, some of the more obvious, and yet fundamental, aspects of our experience of the external world, this is not altogether surprising. The comparatively high degree of development which, as recent historical discoveries have disclosed, it had attained amongst the Babylonians more than five thousand years B.C., may well astonish us. These times must have been preceded by still earlier ages in which the mental evolution of man led him to the use of the tally, and of simple modes of measurement, long before the notions of number and of magnitude appeared in an explicit form.

I have said that mathematics is the oldest of the sciences; a glance at its more recent history will show that it has the energy of perpetual youth. The output of contributions to the advance of the science during the last century and more has been so enormous that it is difficult to say whether pride in the greatness of achievement in his subject, or despair at his inability to cope with the multiplicity of its detailed developments, should be the dominant feeling of the mathematician. Few people outside the small circle of mathematical specialists have any idea of the vast growth of mathematical literature. The Royal Society Catalogue contains a list of nearly thirty-nine thousand papers on subjects of pure mathematics alone, which have appeared in seven hundred serials during the nineteenth century. This represents only a portion of the total output; the very large number of treatises, dissertations and monographs published during the century being omitted. During the first decade of the twentieth century this activity has proceeded at an accelerated rate. Mathematical contributions to mechanics, physics and astronomy would greatly swell the total. A notion of the range of the literature relating not only to

pure mathematics but also to all branches of science to which mathematical methods have been applied will be best obtained by an examination of that monumental work, the "Encyclopädie der mathematischen Wissenschaften"—when it is completed.

The concepts of the pure mathematician, no less than those of the physicist, had their origin in physical experience analyzed and clarified by the reflective activities of the human mind; but the two sets of concepts stand on different planes in regard to the degree of abstraction which is necessary in their formation. Those of the mathematician are more remote from actual analyzed precepts than are those of the physicist, having undergone in their formation a more complete idealization and removal of elements inessential in regard to the purposes for which they are constructed. This difference in the planes of thought frequently gives rise to a certain misunderstanding between the mathematician and the physicist, due in the case of either to an inadequate appreciation of the point of view of the other. On the one hand it is frequently and truly said of particular mathematicians that they are lacking in the physical instinct; and on the other hand a certain lack of sympathy is frequently manifested on the part of physicists for the aims and ideals of the mathematician. The habits of mind and the ideals of the mathematician and of the physicist can not be of an identical character. The concepts of the mathematician necessarily lack, in their pure form, just that element of concreteness which is an essential condition of the success of the physicist, but which to the mathematician would often only obscure those aspects of things which it is his province to study. The abstract mathematical standard of exactitude is one of which the physicist can make no direct use. The calculations in

mathematics are directed towards ideal precision, those in physics consist of approximations within assigned limits of error. The physicist can, for example, make no direct use of such an object as an irrational number; in any given case a properly chosen rational number approximating to the irrational one is sufficient for his purpose. Such a notion as continuity, as it occurs in mathematics, is, in its purity, unknown to the physicist, who can make use only of sensible continuity. The physical counterpart of mathematical discontinuity is very rapid change through a thin layer of transition, or during a very short time. Much of the skill of the true mathematical physicist and of the mathematical astronomer consists in the power of adapting methods and results carried out on an exact mathematical basis to obtain approximations sufficient for the purposes of physical measurement. It might perhaps be thought that a scheme of mathematics on a frankly approximate basis would be sufficient for all the practical purposes of application in physics, engineering science and astronomy; and no doubt it would be possible to develop, to some extent at least, a species of mathematics on these lines. Such a system would, however, involve an intolerable awkwardness and prolixity in the statement of results, especially in view of the fact that the degrees of approximation necessary for various purposes are very different, and thus that unassigned grades of approximation would have to be provided for. Moreover the mathematician working on these lines would be cut off from his chief sources of inspiration, the ideals of exactitude and logical rigor, as well as from one of his most indispensable guides to discovery, symmetry and permanence of mathematical form. The history of the actual movements of mathematical thought through the centuries shows that

these ideals are the very life-blood of the science, and warrants the conclusion that a constant striving towards their attainment is an absolutely essential condition of vigorous growth. These ideals have their roots in irresistible impulses and deep-seated needs of the human mind, manifested in its efforts to introduce intelligibility into certain great domains of the world of thought.

There exists a wide-spread impression amongst physicists, engineers and other men of science that the effect of recent developments of pure mathematics, by making it more abstract than formerly, has been to remove it further from the order of ideas of those who are primarily concerned with the physical world. The prejudice that pure mathematics has its sole *raison d'être* in its function of providing useful tools for application in the physical sciences, a prejudice which did much to retard the due development of pure mathematics in this country during the nineteenth century, is by no means extinct. It is not infrequently said that the present devotion of many mathematicians to the interminable discussion of purely abstract questions relating to modern developments of the notions of number and function, and to theories of algebraic form, serves only the purpose of deflecting them from their proper work into paths which lead nowhere. It is considered that mathematicians are apt to occupy themselves too exclusively with ideas too remote from the physical order in which mathematics had its origin and in which it should still find its proper applications. A direct answer to the question *cui bono?* when it is raised in respect of a department of study such as pure mathematics, seldom carries conviction, in default of a standard of values common to those who ask and to those who answer the question. To appreciate the im-

portance of a sphere of mental activity different from our own always requires some effort of the sympathetic imagination, some recognition of the fact that the absolute value of interests and ideals of a particular class may be much greater than the value which our own mentality inclines us to attach to them. If a defense is needed of the expenditure of time and energy on the abstract problems of pure mathematics, that defense must be of a cumulative character. The fact that abstract mathematical thinking is one of the normal forms of activity of the human mind, a fact which the general history of thought fully establishes, will appeal to some minds as a ground of decisive weight. A great department of thought must have its own inner life, however transcendent may be the importance of its relations to the outside. No department of science, least of all one requiring so high a degree of mental concentration as mathematics, can be developed entirely, or even mainly, with a view to applications outside its own range. The increased complexity and specialization of all branches of knowledge makes it true in the present, however it may have been in former times, that important advances in such a department as mathematics can be expected only from men who are interested in the subject for its own sake, and who, whilst keeping an open mind for suggestions from outside, allow their thought to range freely in those lines of advance which are indicated by the present state of their subject, untrammelled by any preoccupation as to applications to other departments of science. Even with a view to applications, if mathematics is to be adequately equipped for the purpose of coping with the intricate problems which will be presented to it in the future by physics, chemistry and other branches of physical science, many of these

problems probably of a character which we can not at present forecast, it is essential that mathematics should be allowed to develop itself freely on its own lines. Even if much of our present mathematical theorizing turns out to be useless for external purposes, it is wiser, for a well-known reason, to allow the wheat and the tares to grow together. It would be easy to establish in detail that many of the applications which have been actually made of mathematics were wholly unforeseen by those who first developed the methods and ideas on which they rest. Recently, the more refined mathematical methods which have been applied to gravitational astronomy by Delaunay, G. W. Hill, Poincaré, E. W. Brown and others, have thrown much light on questions relating to the solar system, and have much increased the accuracy of our knowledge of the motions of the moon and the planets. Who knows what weapons forged by the theories of functions, of differential equations, or of groups, may be required when the time comes for such an empirical law as Mendeléeff's periodic law of the elements to receive its dynamical explanation by means of an analysis of the detailed possibilities of relatively stable types of motion, the general schematic character of which will have been indicated by the physicist? It is undoubtedly true that the cleft between pure mathematics and physical science is at the present time wider than formerly. That is, however, a result of the natural development, on their own lines, of both subjects. In the classical period of the eighteenth century, the time of Lagrange and Laplace, the nature of the physical investigations, consisting largely of the detailed working out of problems of gravitational astronomy in accordance with Newton's law, was such that the passage was easy from the concrete problems to the cor-

responding abstract mathematical ones. Later on, mathematical physicists were much occupied with problems which lent themselves readily to treatment by means of continuous analysis. In our own time the effect of recent developments of physics has been to present problems of molecular and sub-molecular mechanics to which continuous analysis is not at least directly applicable, and can only be made applicable by a process of averaging the effects of great swarms of discrete entities. The speculative and incomplete character of our conceptions of the structure of the objects of investigation has made the applications of dynamics to their detailed elucidation tentative and partial. The generalized dynamical scheme developed by Lagrange and Hamilton, with its power of dealing with systems, the detailed structure of which is partially unknown, has, however, proved a powerful weapon of attack, and affords a striking instance of the deep-rooted significance of mathematical form. The wonderful and perhaps unprecedentedly rapid discoveries in physics which have been made in the last two decades have given rise to many questions which are as yet hardly sufficiently definite in form to be ripe for mathematical treatment; a necessary condition of which treatment consists in a certain kind of precision in the data of the problems to be solved.

The difficulty of obtaining an adequate notion of the general scope and aims of mathematics, or even of special branches of it, is perhaps greater than in the case of any other science. Many persons, even such as have made a serious and prolonged study of the subject, feel the difficulty of seeing the wood for trees. The severe demands made upon students by the labor of acquiring a difficult technique largely accounts for this; but teachers might do

much to facilitate the attainment of a wider outlook by directing the attention of their students to the more general and less technical aspects of the various parts of the subject, and especially by the introduction into the courses of instruction of more of the historical elements than has hitherto been usual.

All attempts to characterize the domain of mathematics by means of a formal definition which shall not only be complete, but which shall also rigidly mark off that domain from the adjacent provinces of formal logic, on the one side, and of physical science, on the other side, are almost certain to meet with but doubtful success; such success as they may attain will probably be only transient, in view of the power which the science has always shown of constantly extending its borders in unforeseen directions. Such definitions, many of which have been advanced, are apt to err by excess or defect, and often contain distinct traces of the personal predilections of those who formulate them. There was a time when it would have been a tolerably sufficient description of pure mathematics to say that its subject-matter consisted of magnitude and geometrical form. Such a description of it would be wholly inadequate at the present day. Some of the most important branches of modern mathematics, such as the theory of groups, and universal algebra, are concerned, in their abstract forms, neither with magnitude nor with number, nor with geometrical form. That great modern development, projective geometry, has been so formulated as to be independent of all metric considerations. Indeed the tendency of mathematicians under the influence of the movement known as the arithmetization of analysis, a movement which has become a dominant one in the last few decades, is to banish altogether the notion of measur-

able quantity as a conception necessary to pure mathematics; number, in the extended meaning it has attained, taking its place. Measurement is regarded as one of the applications, but as no part of the basis, of mathematical analysis. Perhaps the least inadequate description of the general scope of modern pure mathematics—I will not call it a definition—would be to say that it deals with *form*, in a very general sense of the term; this would include algebraic form, geometrical form, functional relationship, the relations of order in any ordered set of entities such as numbers, and the analysis of the peculiarities of form of groups of operations. A strong tendency is manifested in many of the recent definitions to break down the line of demarcation which was formerly supposed to separate mathematics from formal logic; the rise and development of symbolic logic has no doubt emphasized this tendency. Thus mathematics has been described by the eminent American mathematician and logician B. Peirce as "the science which draws necessary conclusions," a pretty complete identification of mathematics with logical procedure in general. A definition which appears to identify all mathematics with the Mengenlehre, or theory of aggregates, has been given by E. Papperitz: "The subject-matter of pure mathematics consists of the relations that can be established between any objects of thought when we regard those objects as contained in an ordered manifold; the law of order of this manifold must be subject to our choice." The form of definition which illustrates most strikingly the tendencies of the modern school of logistic is one given by Mr. Bertrand Russell. I reproduce it here, in order to show how wide is the chasm between the modes of expression of adherents of this school and those of mathematicians under the influence of

the ordinary traditions of the science. Mr. Russell writes:² "Pure mathematics is the class of all propositions of the form ' p implies q ,' where p and q are propositions containing one or more variables, the same in the two propositions, and neither p nor q contains any constants except logical constants. And logical constants are all notions definable in terms of the following: Implication, the relation of a term to a class of which it is a member, the notion of *such that*, the notion of relation, and such further notions as may be involved in the general notion of propositions of the above form. In addition to these, mathematics *uses* a notion which is not a constituent of the propositions which it considers—namely, the notion of truth."

The belief is very general amongst instructed persons that the truths of mathematics have absolute certainty, or at least that there appertains to them the highest degree of certainty of which the human mind is capable. It is thought that a valid mathematical theorem is necessarily of such a character as to compel belief in any mind capable of following the steps of the demonstration. Any considerations tending to weaken this belief would be disconcerting and would cause some degree of astonishment. At the risk of this, I must here mention two facts which are of considerable importance as regards an estimation of the precise character of mathematical knowledge. In the first place, it is a fact that frequently, and at various times, differences of opinion have existed among mathematicians, giving rise to controversies as to the validity of whole lines of reasoning, and affecting the results of such reasoning; a considerable amount of difference of opinion of this character exists among mathematicians at the present time. In the second place, the accepted

² "Principles of Mathematics," p. 1.

standard of rigor, that is, the standard of what is deemed necessary to constitute a valid demonstration, has undergone change in the course of time. Much of the reasoning which was formerly regarded as satisfactory and irrefutable is now regarded as insufficient to establish the results which it was employed to demonstrate. It has even been shown that results which were once supposed to have been fully established by demonstrations are, in point of fact, affected with error. I propose here to explain in general terms how these phenomena are possible.

In every subject of study, if one probes deep enough, there are found to be points in which that subject comes in contact with general philosophy, and where differences of philosophical view will have a greater or less influence on the attitude of the mind towards the principles of the particular subject. This is not surprising when we reflect that there is but one universe of thought, that no department of knowledge can be absolutely isolated, and that metaphysical and psychological implications are a necessary element in all the activities of the mind. A particular department, such as mathematics, is compelled to set up a more or less artificial frontier, which marks it off from general philosophy. This frontier consists of a set of regulative ideas in the form of indefinables and axioms, partly ontological assumptions, and partly postulations of a logical character. To go behind these, to attempt to analyze their nature and origin, and to justify their validity, is to go outside the special department and to touch on the domains of the metaphysician and the psychologist. Whether they are regarded as possessing apodictic certainty or as purely hypothetical in character, these ideas represent the data or premises of the science, and the whole of its edifice is de-

pended upon them. They serve as the foundation on which all is built, as well as the frontier on the side of philosophy and psychology. A set of data ideally perfect in respect of precision and permanence is unattainable—or at least has not yet been attained; and the adjustment of frontiers is one of the most frequent causes of strife. As a matter of fact, variations of opinion have at various times arisen within the ranks of the mathematicians as to the nature, scope and proper formulation of the principles which form the foundations of the science, and the views of mathematicians in this regard have always necessarily been largely affected by the conscious or unconscious attitude of particular minds towards questions of general philosophy. It is in this region, I think, that the source is to be found of those remarkable differences of opinion amongst mathematicians which have come into prominence at various times, and have given rise to much controversy as to fundamentals. Since the time of Newton and Leibnitz there has been almost unceasing discussion as to the proper foundations for the so-called infinitesimal calculus. More recently, questions relating to the foundations of geometry and rational mechanics have much occupied the attention of mathematicians. The very great change which has taken place during the last half century in the dominant view of the foundations of mathematical analysis—a change which has exercised a great influence extending through the whole detailed treatment of that subject—although critical in its origin, has been constructive in its results. The Mengenlehre, or theory of aggregates, had its origin in the critical study of the foundations of analysis, but has already become a great constructive scheme, is indispensable as a method in the investigations of analysis, provides the

language requisite for the statement in precise form of analytical theorems of a general character, and, moreover, has already found important applications in geometry. In connection with the Mengenlehre there has arisen a controversy amongst mathematicians which is at the present time far from having reached a decisive issue. The exact point at issue is one which may be described as a matter of mathematical ontology; it turns upon the question of what constitutes a valid definition of a mathematical object. The school known as mathematical "idealists" admit, as valid objects of mathematical discussion, entities which the rival "empiricist" school regard as non-existent for mathematical thought, because insufficiently defined. It is clear that the idealist may build whole superstructures on a foundation which the empiricist regards as made of sand, and this is what has actually happened in some of the recent developments of what has come to be known as Cantorism. The difference of view of these rival schools, depending as it does on deep-seated differences of philosophical outlook, is thought by some to be essentially irreconcileable. This controversy was due to the fact that certain processes of reasoning, of very considerable plausibility, which had been employed by G. Cantor, the founder of the Mengenlehre, had led to results which contained flat contradictions. The efforts made to remove these contradictions, and to trace their source, led to the discussion, disclosing much difference of opinion, of the proper definitions and principles on which the subject should be based.

The proposition $7 + 5 = 12$, taken as typical of the propositions expressing the results of the elementary operations of arithmetic, has since the time of Kant given rise to very voluminous discussion amongst

philosophers, in relation to the precise meaning and implication of the operation and the terms. It will, however, be maintained, probably by the majority of mankind, that the theorem retains its validity as stating a practically certain and useful fact, whatever view philosophers may choose to take of its precise nature—as, for example, whether it represents, in the language of Kant, a synthetic or an analytic judgment. It may, I think, be admitted that there is much cogency in this view; and, were mathematics concerned with the elementary operations of arithmetic alone, it could fairly be held that the mathematician, like the practical man of the world, might without much risk shut his eyes and ears to the discussions of the philosophers on such points. The exactitude of such a proposition, in a sufficiently definite sense for practical purposes, is empirically verifiable by sensuous intuition, whatever meaning the metaphysician may attach to it. But mathematics can not be built up from the operations of elementary arithmetic without the introduction of further conceptual elements. Except in certain very simple cases no process of measurement, such as the determination of an area or a volume, can be carried out with exactitude by a finite number of applications of the operations of arithmetic. The result to be obtained appears in the form of a limit, corresponding to an interminable sequence of arithmetical operations. The notion of "limit," in the definite form given to it by Cauchy and his followers, together with the closely related theory of the arithmetic continuum, and the notions of continuity and functionality, lie at the very heart of modern analysis. Essentially bound up with this central doctrine of limits is the concept of a non-finite set of entities, a concept which is not directly derivable from sensuous intuition, but which is never-

theless a necessary postulation in mathematical analysis. The conception of the infinite, in some form, is thus indispensable in mathematics; and this conception requires precise characterization by a scheme of exact definitions, prior to all the processes of deduction required in obtaining the detailed results of analysis. The formulation of this precise scheme gives an opening to differences of philosophical opinion which has led to a variety of views as to the proper character of those definitions which involve the concept of the infinite. Here is the point of divergence of opinion among mathematicians to which I have alluded above. Under what conditions is a non-finite aggregate of entities a properly defined object of mathematical thought, of such a character that no contradictions will arise in the theories based upon it? That is the question to which varying answers have been offered by different mathematical thinkers. No one answer of a completely general character has as yet met with universal acceptance. Physical intuition offers no answer to such a question; it is one which abstract thought alone can settle. It can not be altogether avoided, because, without the notion of the infinite, at least in connection with the central conception of the "limit," mathematical analysis as a coherent body of thought falls to the ground.

Both in geometry and in analysis our standard of what constitutes a rigorous demonstration has in the course of the nineteenth century undergone an almost revolutionary change. That oldest textbook of science in the world, "Euclid's Elements of Geometry," has been popularly held for centuries to be the very model of deductive logical demonstration. Criticism has, however, largely invalidated this view. It appears that, at a large number of points, assumptions not included in

the preliminary axioms and postulates are made use of. The fact that these assumptions usually escape notice is due to their nature and origin. Derived as they are from our spatial intuition, their very self-evidence has allowed them to be ignored, although their truth is not more obvious empirically than that of other assumptions derived from the same source which are included in the axioms and postulates explicitly stated as part of the foundation of Euclid's treatment of the subject. The method of superimposition, employed by Euclid with obvious reluctance, but forming an essential part of his treatment of geometry, is, when regarded from his point of view, open to most serious objections as regards its logical coherence. In analysis, as in geometry, the older methods of treatment consisted of processes of deduction eking out by the more or less surreptitious introduction, at numerous points in the subject, of assumptions only justifiable by spatial intuition. The result of this deviation from the purely deductive method was more disastrous in the case of analysis than in geometry, because it led to much actual error in the theory. For example, it was held until comparatively recently that a continuous function necessarily possesses a differential coefficient, on the ground that a curve always has a tangent. This we now know to be quite erroneous, when any reasonable definition of continuity is employed. The first step in the discovery of this error was made when it occurred to Ampère that the existence of the differential coefficient could only be asserted as a theorem requiring proof; and he himself published an attempt at such proof. The erroneous character of the former belief on this matter was most strikingly exhibited when Weierstrass produced a function which is everywhere continuous, but which nowhere possesses a differential coefficient;

such functions can now be constructed *ad libitum*. It is not too much to say that no one of the general theorems of analysis is true without the introduction of limitations and conditions which were entirely unknown to the discoverers of those theorems. It has been the task of mathematicians under the lead of such men as Cauchy, Riemann, Weierstrass and G. Cantor, to carry out the work of reconstruction of mathematical analysis, to render explicit all the limitations of the truth of the general theorems, and to lay down the conditions of validity of the ordinary analytical operations. Physicists and others often maintain that this modern extreme precision amounts to an unnecessary and pedantic purism, because in all practical applications of mathematics only such functions are of importance as exclude the remoter possibilities contemplated by theorists. Such objections leave the true mathematician unmoved; to him it is an intolerable defect that, in an order of ideas in which absolute exactitude is the guiding ideal, statements should be made, and processes employed, both of which are subject to unexpressed qualifications, as conditions of their truth or validity. The pure mathematician has developed a specialized conscience, extremely sensitive as regards sins against logical precision. The physicist, with his conscience hardened in this respect by the rough-and-tumble work of investigating the physical world, is apt to regard the more tender organ of the mathematician with that feeling of impatience, not unmixed with contempt, which the man of the world manifests for what he considers to be over-scrupulousity and unpracticality.

It is true that we can not conceive how such a science as mathematics could have come into existence apart from physical experience. But it is also true that phys-

ical precepts, as given directly in unanalyzed experience, are wholly unfitted to form the basis of an exact science. Moreover, physical intuition fails altogether to afford any trustworthy guidance in connection with the concept of the infinite, which, as we have seen, is in some form indispensable in the formation of a coherent system of mathematical analysis. The hasty and uncritical extension to the region of the infinite, of results which are true and often obvious in the region of the finite, has been a fruitful source of error in the past, and remains as a pitfall for the unwary student in the present. The notions derived from physical intuition must be transformed into a scheme of exact definitions and axioms before they are available for the mathematician, the necessary precision being contributed by the mind itself. A very remarkable fact in connection with this process of refinement of the rough data of experience is that it contains an element of arbitrariness, so that the result of the process is not necessarily unique. The most striking example of this want of uniqueness in the conceptual scheme so obtained is the case of geometry, in which it has been shown to be possible to set up various sets of axioms, each set self-consistent, but inconsistent with any other of the sets, and yet such that each set of axioms, at least under suitable limitations, leads to results consistent with our perception of actual space-relations. Allusion is here made, in particular, to the well-known geometries of Lobatchewsky and of Riemann, which differ from the geometry of Euclid in respect of the axiom of parallels, in place of which axioms inconsistent with that of Euclid and with one another are substituted. It is a matter of demonstration that any inconsistency which might be supposed to exist in the scheme known as hyperbolic geometry, or in that known

as elliptic geometry, would necessarily entail the existence of a corresponding inconsistency in Euclid's set of axioms. The three geometries, therefore, from the logical point of view, are completely on a par with one another. An interesting mathematical result is that all efforts to prove Euclid's axiom of parallels, *i. e.*, to deduce it from his other axioms, are doomed to necessary failure; this is of importance in view of the many efforts that have been made to obtain the proof referred to. When the question is raised which of these geometries is the true one, the kind of answer that will be given depends a good deal on the view taken of the relation of conceptual schemes in general to actual experience. It is maintained by M. Poincaré, for example, that the question which is the true scheme has no meaning; that it is, in fact, entirely a matter of convention and convenience which of these geometries is actually employed in connection with spatial measurements. To decide between them by a crucial test is impossible, because our space perceptions are not sufficiently exact in the mathematical sense to enable us to decide between the various axioms of parallels. Whatever views are taken as to the difficult questions that arise in this connection, the contemplation and study of schemes of geometry wider than that of Euclid, and some of them including Euclid's geometry as a special case, is of great interest not only from the purely mathematical point of view, but also in relation to the general theory of knowledge, in that, owing to the results of this study, some change is necessitated in the views which have been held by philosophers as to what is known as Kant's space-problem.

The school of thought which has most emphasized the purely logical aspect of mathematics is that which is represented in this country by Mr. Bertrand Russell and

Dr. Whitehead, and which has distinguished adherents both in Europe and in America. The ideal of this school is a presentation of the whole of mathematics as a deductive scheme in which are employed a certain limited number of indefinables and unprovable axioms, by means of a procedure in which all possibility of the illicit intrusion of extraneous elements into the deduction is excluded by the employment of a symbolism in which each symbol expresses a certain logical relation. This school receives its inspiration from a peculiar form of philosophic realism which, in its revolt from idealism, produces in the adherents of the school a strong tendency to ignore altogether the psychological implications in the movements of mathematical thought. This is carried so far that in their writings no explicit recognition is made of any psychological factors in the selection of the indefinables and in the formulation of the axioms upon which the whole structure of mathematics is to be based. The actually worked-out part of their scheme has as yet reached only the mere fringe of modern mathematics as a great detailed body of doctrine; but to any objection to the method on the ground of the prolixity of the treatment which would be necessary to carry it out far enough to enable it to embrace the various branches of mathematics in all the wealth of their present development, it would probably be replied that the main point of interest is to establish in principle the possibility only of subsuming pure mathematics under a scheme of logistics. It is quite impossible for me here to attempt to discuss, even in outline, the tenets of this school, or even to deal with the interesting question of the possibility of setting up a final system of indefinables and axioms which shall suffice for all present and future developments of mathematics.

I am very far from wishing to minimize the high philosophic interest of the attempt made by the Peano-Russell school to exhibit mathematics as a scheme of deductive logic. I have myself emphasized above the necessity and importance of fitting the results of mathematical research in their final form into a framework of deduction, for the purpose of ensuring the complete precision and the verification of the various mathematical theories. At the same time it must be recognized that the purely deductive method is wholly inadequate as an instrument of research. Whatever view may be held as regards the place of psychological implications in a completed body of mathematical doctrine, in research the psychological factor is of paramount importance. The slightest acquaintance with the history of mathematics establishes the fact that discoveries have seldom, or never, been made by purely deductive processes. The results are thrown into a purely deductive form after, and often long after, their discovery. In many cases the purely deductive form, in the full sense, is quite modern. The possession of a body of indefinables, axioms or postulates, and symbols denoting logical relation, would, taken by itself, be wholly insufficient for the development of a mathematical theory. With these alone the mathematician would be unable to move a step. In face of an unlimited number of possible combinations a principle of selection of such as are of interest, a purposive element, and a perceptive faculty are essential for the development of anything new. In the process of discovery the chains in a sequence of logical deduction do not at first arise in their final order in the mind of the mathematical discoverer. He divines the results before they are established; he has an intuitive grasp of the general line of a demonstration long before he has filled in the details. A developed theory, or even a demonstration of a single theorem, is no

more identical with a mere complex of syllogisms than a melody is identical with the mere sum of the musical notes employed in its composition. In each case the whole is something more than merely the sum of its parts; it has a unity of its own, and that unity must be, in some measure at least, discerned by its creator before the parts fall completely into their places. Logic is, so to speak, the grammar of mathematics; but a knowledge of the rules of grammar and the letters of the alphabet would not be sufficient equipment to enable a man to write a book. There is much room for individuality in the modes of mathematical discovery. Some great mathematicians have employed largely images derived from spatial intuition as a guide to their results; others appear wholly to have discarded such aids, and were led by a fine feeling for algebraic and other species of mathematical form. A certain tentative process is common, in which, by the aid of results known or obtained in special cases, generalizations are perceived and afterwards established, which take up into themselves all the special cases so employed. Most mathematicians leave some traces, in the final presentation of their work, of the scaffolding they have employed in building their edifices: some much more than others.

The difference between a mathematical theory in the making and as a finished product is, perhaps, most strikingly illustrated by the case of geometry, as presented in its most approved modern shape. It is not too much to say that geometry, reduced to a purely deductive form—as presented, for example, by Hilbert, or by some of the modern Italian school—has no necessary connection with space. The words “point,” “line,” “plane” are employed to denote any entities whatever which satisfy certain prescribed conditions of rela-

tionship. Various premises are postulated that would appear to be of a perfectly arbitrary nature, if we did not know how they had been suggested. In that division of the subject known as metric geometry, for example, axioms of congruency are assumed which, by their purely abstract character, avoid the very real difficulties that arise in this regard in reducing perceptual space-relations of measurements to a purely conceptual form. Such schemes, triumphs of constructive thought at its highest and most abstract level as they are, could never have been constructed apart from the space-perceptions that suggested them, although the concepts of spatial origin are transformed almost out of recognition. But what I want to call attention to here is that, apart from the basis of this geometry, mathematicians would never have been able to find their way through the details of the deductions without having continual recourse to the guidance given them by spatial intuition. If one attempts to follow one of the demonstrations of a particular theorem in the work of writers of this school, one would find it quite impossible to retain the steps of the process long enough to master the whole, without the aid of the very spatial suggestions which have been abstracted. This is perhaps sufficiently warranted by the fact that writers of this school find it necessary to provide their readers with figures, in order to avoid complete bewilderment in following the demonstrations, although the processes, being purely logical deductions from premises of the nature I have described, deal only with entities which have no necessary similarity to anything indicated by the figures.

A most interesting account has been written by one of the greatest mathematicians of our time, M. Henri Poincaré, of the way in which he was led to some of

his most important mathematical discoveries.³ He describes the process of discovery as consisting of three stages: the first of these consists of a long effort of concentrated attention upon the problem in hand in all its bearings; during the second stage he is not consciously occupied with the subject at all, but at some quite unexpected moment the central idea which enables him to surmount the difficulties, the nature of which he had made clear to himself during the first stage, flashes suddenly into his consciousness. The third stage consists of the work of carrying out in detail and reducing to a connected form the results to which he is led by the light of his central idea; this stage, like the first, is one requiring conscious effort. This is, I think, clearly not a description of a purely deductive process; it is assuredly more interesting to the psychologist than to the logician. We have here the account of a complex of mental processes in which it is certain that the reduction to a scheme of precise logical deduction is the latest stage. After all, a mathematician is a human being, not a logic-engine. Who that has studied the works of such men as Euler, Lagrange, Cauchy, Riemann, Sophus Lie and Weierstrass can doubt that a great mathematician is a great artist? The faculties possessed by such men, varying greatly in kind and degree with the individual, are analogous to those requisite for constructive art. Not every great mathematician possesses in a specially high degree that critical faculty which finds its employment in the perfection of form, in conformity with the ideal of logical completeness; but every great mathematician possesses the rarer faculty of constructive imagination.

The actual evolution of mathematical theories proceeds by a process of induction

³ See the *Revue du Mois* for 1908.

strictly analogous to the method of induction employed in building up the physical sciences; observation, comparison, classification, trial and generalization are essential in both cases. Not only are special results, obtained independently of one another, frequently seen to be really included in some generalization, but branches of the subject which have been developed quite independently of one another are sometimes found to have connections which enable them to be synthesized in one single body of doctrine. The essential nature of mathematical thought manifests itself in the discernment of fundamental identity in the mathematical aspects of what are superficially very different domains. A striking example of this species of immanent identity of mathematical form was exhibited by the discovery of that distinguished mathematician, our general secretary, Major MacMahon, that all possible Latin squares are capable of enumeration by the consideration of certain differential operators. Here we have a case in which an enumeration, which appears to be not amenable to direct treatment, can actually be carried out in a simple manner when the underlying identity of the operation is recognized with that involved in certain operations due to differential operators, the calculus of which belongs superficially to a wholly different region of thought from that relating to Latin squares. The modern abstract theory of groups affords a very important illustration of this point; all sets of operations, whatever be their concrete character, which have the same group, are, from the point of view of the abstract theory, identical, and an analysis of the properties of the abstract group gives results which are applicable to all the actual sets of operations, however diverse their character, which are dominated by the one group. The characteristic feature

of any special geometrical scheme is known when the group of transformations which leave unaltered certain relations of figures has been assigned. Two schemes in which the space elements may be quite different have this fundamental identity, provided they have the same group; every special theorem is then capable of interpretation as a property of figures either in the one or in the other geometry. The mathematical physicist is familiar with the fact that a single mathematical theory is often capable of interpretation in relation to a variety of physical phenomena. In some instances a mathematical formulation, as in some fashion representing observed facts, has survived the physical theory it was originally devised to represent. In the case of electromagnetic and optical theory, there appears to be reason for trusting the equations, even when the proper physical interpretation of some of the vectors appearing in them is a matter of uncertainty and gives rise to much difference of opinion; another instance of the fundamental nature of mathematical form.

One of the most general mathematical conceptions is that of functional relationship, or "functionality." Starting originally from simple cases such as a function represented by a power of a variable, this conception has, under the pressure of the needs of expanding mathematical theories, gradually attained the completeness of generality which it possesses at the present time. The opinion appears to be gaining ground that this very general conception of functionality, born on mathematical ground, is destined to supersede the narrower notion of causation, traditional in connection with the natural sciences. As an abstract formulation of the idea of determination in its most general sense, the notion of functionality includes and transcends the more special notion of causation

as a one-sided determination of future phenomena by means of present conditions; it can be used to express the fact of the subsumption under a general law of past, present and future alike, in a sequence of phenomena. From this point of view the remark of Huxley that mathematics "knows nothing of causation" could only be taken to express the whole truth, if by the term "causation" is understood "efficient causation." The latter notion has, however, in recent times been to an increasing extent regarded as just as irrelevant in the natural sciences as it is in mathematics; the idea of thoroughgoing determinancy, in accordance with formal law, being thought to be alone significant in either domain.

The observations I have made in the present address have, in the main, had reference to mathematics as a living and growing science related to and permeating other great departments of knowledge. The small remaining space at my disposal I propose to devote to a few words about some matters connected with the teaching of the more elementary parts of mathematics. Of late years a new spirit has come over the mathematical teaching in many of our institutions, due in no small measure to the reforming zeal of our general treasurer, Professor John Perry. The changes that have been made followed a recognition of the fact that the abstract mode of treatment of the subject that had been traditional was not only wholly unsuitable as a training for physicists and engineers, but was also to a large extent a failure in relation to general education, because it neglected to bring out clearly the bearing of the subject on the concrete side of things. With the general principle that a much less abstract mode of treatment than was formerly customary is desirable for a variety of reasons, I am in

complete accord. It is a sound educational principle that instruction should begin with the concrete side, and should only gradually introduce the more general and abstract aspects of the subject; an abstract treatment on a purely logical basis being reserved only for that highest and latest stage which will be reached only by a small minority of students. At the same time I think there are some serious dangers connected with the movement towards making the teaching of mathematics more practical than formerly, and I do not think that, in making the recent changes in the modes of teaching, these dangers have always been successfully avoided.

Geometry and mechanics are both subjects with two sides: on the one side, the observational, they are physical sciences; on the other side, the abstract and deductive, they are branches of pure mathematics. The older traditional treatment of these subjects has been of a mixed character, in which deduction and induction occurred side by side throughout, but far too much stress was laid upon the deductive side, especially in the earlier stages of instruction. It is the proportion of the two elements in the mixture that has been altered by the changed methods of instruction of the newer school of teachers. In the earliest teaching of the subjects they should, I believe, be treated wholly as observational studies. At a later stage a mixed treatment must be employed, observation and deduction going hand in hand, more stress being, however, laid on the observational side than was formerly customary. This mixed treatment leaves much opening for variety of method; its character must depend to a large extent on the age and general mental development of the pupils; it should allow free scope for the individual methods of various teachers as suggested to those teachers by experi-

ence. Attempts to fix too rigidly any particular order of treatment of these subjects are much to be deprecated, and, unfortunately, such attempts are now being made. To have escaped from the thraldom of Euclid will avail little if the study of geometry in all the schools is to fall under the domination of some other rigidly prescribed scheme.

There are at the present time some signs of reaction against the recent movement of reform in the teaching of geometry. It is found that the lack of a regular order in the sequence of propositions increases the difficulty of the examiner in appraising the performance of the candidates, and in standardizing the results of examinations. That this is true may well be believed, and it was indeed foreseen by many of those who took part in bringing about the de-thronement of Euclid as a text-book. From the point of view of the examiner it is without doubt an enormous simplification if all the students have learned the subject in the same order, and have studied the same text-book. But, admitting this fact, ought decisive weight to be allowed to it? I am decidedly of opinion that it ought not. I think the convenience of the examiner, and even precision in the results of examinations, ought unhesitatingly to be sacrificed when they are in conflict—as I believe they are in this case—with the vastly more important interests of education. Of the many evils which our examination system has inflicted upon us, the central one has consisted in forcing our school and university teaching into moulds determined not by the true interests of education, but by the mechanical exigencies of the examination syllabus. The examiner has thus exercised a potent influence in discouraging initiative and individuality of method on the part of the teacher; he has robbed the teacher of that free-

dom which is essential for any high degree of efficiency. An objection of a different character to the newer modes of teaching geometry has been frequently made of late. It is said that the students are induced to accept and reproduce, as proofs of theorems, arguments which are not really proofs, and thus that the logical training which should be imparted by a study of geometry is vitiated. If this objection really implies a demand for a purely deductive treatment of the subject, I think some of those who raise it hardly realize all that would be involved in the complete satisfaction of their requirement. I have already remarked that Euclid's treatment of the subject is not rigorous as regards logic. Owing to the recent exploration of the foundations of geometry we possess at the present time tolerably satisfactory methods of purely deductive treatment of the subject; in regard to mechanics, notwithstanding the valuable work of Mach, Herz and others, this is not yet the case. But, in the schemes of purely deductive geometry, the systems of axioms and postulates are far from being of a very simple character; their real nature, and the necessity for many of them, can only be appreciated at a much later stage in mathematical education than the one of which I am speaking. A purely logical treatment is the highest stage in the training of the mathematician, and is wholly unsuitable—and, indeed, quite impossible—in those stages beyond which the great majority of students never pass. It can then, in the case of all students, except a few advanced ones in the universities, only be a question of degree how far the purely logical factor in the proofs of propositions shall be modified by the introduction of elements derived from observation or spatial intuition. If the freedom of teaching which I have advocated be allowed, it

will be open to those teachers who find it advisable in the interests of their students to emphasize the logical side of their teaching to do so; and it is certainly of value in all cases to draw the attention of students to those points in a proof where the intuitive element enters. I draw, then, the conclusion that a mixed treatment of geometry, as of mechanics, must prevail in the future, as it has done in the past, but that the proportion of the observational or intuitive factor to the logical one must vary in accordance with the needs and intellectual attainments of the students, and that a large measure of freedom of judgment in this regard should be left to the teacher.

The great and increasing importance of a knowledge of the differential and integral calculus for students of engineering and other branches of physical science has led to the publication during the last few years of a considerable number of text-books on this subject intended for the use of such students. Some of these text-books are excellent, and their authors, by a skilful insistence on the principles of the subject, have done their utmost to guard against the very real dangers which attend attempts to adapt such a subject to the practical needs of engineers and others. It is quite true that a great mass of detail which has gradually come to form part—often much too large a part—of the material of the student of mathematics, may with great advantage be ignored by those whose main study is to be engineering science or physics. Yet it cannot be too strongly insisted on that a firm grasp of the principles, as distinct from the mere processes of calculation, is essential if mathematics is to be a tool really useful to the engineer and the physicist. There is a danger, which experience has shown to be only too real, that such students may learn

to regard mathematics as consisting merely of formulæ and of rules which provide the means of performing the numerical computations necessary for solving certain categories of problems which occur in the practical sciences. Apart from the deplorable effect, on the educational side, of degrading mathematics to this level, the practical effect of reducing it to a number of rule-of-thumb processes can only be to make those who learn it in so unintelligent a manner incapable of applying mathematical methods to any practical problem in which the data differ even slightly from those in the model problems which they have studied. Only a firm grasp of the principles will give the necessary freedom in handling the methods of mathematics required for the various practical problems in the solution of which they are essential.

E. W. HOBSON.

GRANTS BY THE BRITISH ASSOCIATION

At the Sheffield meeting of the British Association the sum of £1,090 was appropriated for scientific work, the grants being as follows:

Mathematical and Physical Science

Turner, Professor H. H.—Seismological Observations	£60
Shaw, Dr. W. N.—Upper Atmosphere	25
Preece, Sir W. H.—Magnetic Observations at Falmouth	25
Gill, Sir David—Establishing a Solar Observatory in Australia	50
Gill, Sir David—Grant to the International Commission on Physical and Chemical Constants	30

Chemistry

Divers, Professor E.—Study of Hydro-aromatic Substances	20
Armstrong, Professor H. E.—Dynamic Isomerism	25
Kipping, Professor F. S.—Transformation of Aromatic Nitroamines	15
Kipping, Professor F. S.—Electro-analysis ..	15
Arnold, Professor J. O.—Influence of Carbon, etc., on Corrosion of Steel	15

Geology

Harker, Dr. A.—Crystalline Rocks of Anglesey	2
Tiddeman, R. H.—Erratic Blocks	10
Lapworth, Professor C.—Paleozoic Rocks	10
Watts, Professor W. W.—Composition of Charnwood Rocks	2
Watts, Professor W. W.—Igneous and Associated Sedimentary Rocks of Glensaul	15
Bourne, Professor G. C.—Mammalian Fauna in Miocene Deposits, Bugti Hills, Baluchistan	45

Zoology

Woodward, Dr. H.—Index Animalium	75
Hickson, Professor S. J.—Table at the Zoological Station at Naples	75
Shipley, Dr. A. E.—Feeding Habits of British Birds	5
Shipley, Dr. A. E.—Belmullet Whaling Station	30
Bourne, Professor G. C.—Mammalian Fauna in Miocene Deposits, Bugti Hills, Baluchistan	30

Geography

Chisholm, G. G.—Map of Prince Charles Foreland	30
Herbertson, Professor A. J.—Equal Area Maps	20
Cannan, Professor E.—Amount and Distribution of Income	5

Engineering

Preece, Sir W. H.—Gaseous Explosions	90
--	----

Anthropology

Munro, Dr. R.—Glastonbury Lake Village ..	5
Myres, Professor J. L.—Excavations on Roman Sites in Britain	10
Read, C. H.—Age of Stone Circles	30
Read, C. H.—Anthropological Notes and Queries	40
Munro, Dr. R.—Artificial Islands in Highland Lochs	10

Physiology

Schäfer, Professor E. A.—The Ductless Glands	40
Sherrington, Professor C. S.—Body Metabolism in Cancer	6
Hickson, Professor S. J.—Table at the Zoological Station at Naples	25
Waller, Professor A. D.—Electromotive Phenomena in Plants	10

Waller, Professor A. D.—Anesthetics	20
Sherrington, Professor C. S.—Mental and Muscular Fatigue	25
Starling, Professor E. H.—Dissociation of Oxyhemoglobin	25
<i>Botany</i>	
Scott, Dr. D. H.—Structure of Fossil Plants	15
Darwin, Dr. F.—Experimental Study of Heredity	45
Johnson, Professor T.—Survey of Clare Island	20
Oliver, Professor F. W.—Registration of Bot- anical Photographs	10
<i>Education</i>	
Findlay, Professor J. J.—Mental and Phys- ical Factors	10
<i>Corresponding Societies Committee</i>	
Whitaker, W.—For Preparation of Report ..	20

SCIENTIFIC NOTES AND NEWS

SIR WILLIAM RAMSAY has been elected president of the British Association for the meeting to be held next year at Portsmouth. The meeting of 1912 will be at Dundee. The meeting of 1914 will be held in Australia in the cities of Adelaide, Melbourne, Sidney and Brisbane. The commonwealth government has voted £10,000 toward the expenses of the meeting, and the several states will make additional contributions.

THE Accademia dei Lincei of Rome has elected foreign members as follows: E. G. van de Sande Bakhuyzen in astronomy; John Henry Poynting for physics; Armand Gautier in chemistry; Wilhelm Waldeyer and Richard Hertwig for zoology and morphology, and Max Verworn and Ludimar Hermann for physiology.

DR. WILLY WIEN, professor of physics in the University of Würzburg; Dr. Felix Marchand, professor of pathology at the University of Leipzig; Dr. Friedrich Merkel, professor of anatomy at the University of Göttingen; Dr. Gustav Schwalbe, professor of anatomy at the University of Strasburg, and Dr. Oswald Schmiedeberg, professor of pharmacology at the University of Strasburg, have been elected corresponding members of the Berlin Academy of Sciences.

PROFESSOR CZERNY will preside over the second International Conference for the Study of Cancer to be held in Paris from October 1 to 5.

AT the thirty-eighth annual meeting of the American Public Health Association, held in Milwaukee, September 6 to 9, Dr. Robert M. Simpson, of Winnipeg, Man., was elected president.

DR. HERMAN A. SPOEHR, assistant in chemistry in the University of Chicago, has been appointed a member of the staff of the department of botanical research of the Carnegie Institution of Washington. Dr. Spoehr is investigating certain problems in plant physiology which lend themselves to the application of chemical methods.

MR. D. P. ROBERTS, electrical engineer at London, Ont., Canada, has been appointed electrical expert and inspector for the British Columbia government.

SEVERAL collections of bees from the British Museum and from the Berlin Museum were classified by Professor Theodore D. A. Cockerell at the request of those institutions this summer. During the latter part of the summer he has been working on a collection of fossils sent to him for classification by the American Museum of Natural History of New York.

DEAN MILO S. KETCHUM returns to the University of Colorado after a year's leave of absence. As a member of the firm, Crocker and Ketchum, consulting engineers, Denver, he has been designing and constructing bridges, viaducts, dams and reinforced concrete structures. He is now consulting engineer for the Albion dam which the city of Boulder is to build.

PROFESSOR JOSEPHINE TILDEN, of the department of botany of the University of Minnesota, who spent the year in New Zealand studying the algae of the southern Pacific, has returned to the university.

It is proposed to name the new hospital for contagious diseases at Buffalo after the late Dr. Ernest Wende, as a memorial of his services as a sanitarian and health officer.

PROFESSOR W. KOLLE has been appointed head of the newly erected laboratory of hygiene and bacteriology at Berne.

THE centenary of the death of the Italian naturalist Filippo Cavolini will be commemorated by a series of meetings to be held in Naples beginning on September 12.

DR. JAMES NEVINS HYDE, of Chicago, for thirty-one years professor of dermatology in the Rush Medical College, died on September 6.

PROFESSOR EDOUARD HEINRICH HENOCH, one of the founders of modern pediatrics, died at Dresden on August 22, in his ninety-first year.

PROFESSOR VON RECKLINGHAUSEN, since 1872 professor of pathology at Strasburg, died on August 26 at the age of seventy-six years.

THE death is announced of Professor Pedroso, president of the Geographical Society of Lisbon.

THE ninth International Conference on Tuberculosis will take place at Brussels on October 5-8. *Nature* states that among the subjects likely to be brought under consideration are: Hereditary tuberculosis contagion; the pre-disposition to the disease; the protection of children against tuberculosis; tuberculosis and the school; the part of women in the campaign against tuberculosis. Reports on the progress of the war against tuberculosis in different countries, milk supply, solar radiation, international statistics and international marks indicating the condition of the lungs will be presented, and a paper will be read by Dr. Nathan Raw on the general measures recommended by the International Conference to the public authorities for the prevention of the spread of tuberculosis in different countries.

THE first mid-summer meeting of Illinois farmers was held in August at the University of Illinois. The purpose of holding this institute in the middle of the summer was to give an opportunity to farmers and others interested in agriculture to see the agricultural experiment plots, the oldest in the United States, at a time when they were bearing crops. The institute was attended by about 2,500 farmers, bankers, teachers and various

other professional men who were interested in agriculture. One of the leading addresses delivered at this meeting was by N. Kaumans, German commissioner for agriculture. The main idea in the address was the necessity of the conservation of the soil. Commissioner Kaumans was merely emphasizing, however, what Professor Cyril G. Hopkins, the head of the department of agronomy at the university, and others have been saying for a number of years.

THE following arrangements are given in *Nature* for the opening of the winter session of the London medical schools: St. George's Hospital, King's College Hospital and London Hospital will open on October 1. At the first-named Dr. S. Squire Sprigge will deliver an oration "On Prizes." St. Bartholomew's Hospital, Charing Cross Hospital (at which Dr. F. W. Mott, F.R.S., will deliver the eighth Huxley lecture, on "The Hereditary Aspect of Nervous and Mental Diseases"), Guy's Hospital, London (Royal Free Hospital), School of Medicine for Women (at which an address on "Women's Sphere in Medicine" will be given by Dr. E. W. Roughton), Middlesex Hospital, St. Mary's Hospital, University College Hospital and Westminster Hospital will reopen on October 3. The opening day for St. Thomas's Hospital is October 4, and that of the London School of Tropical Medicine is October 14, when Dr. H. A. Miers, F.R.S., will give an address. At the opening of the medical school of the Victoria University of Manchester, on October 3, Professor W. Thorburn will speak on "The Evolution of Surgery."

THE availability of even low-grade phosphate rock for use as a fertilizer gives importance to the enormous phosphate deposits in Idaho, Wyoming and Utah, many of which are on government land. The total area of public phosphate lands now withheld from entry is more than two and a half million acres. Portions of the lands thus withdrawn were examined in 1909 by geologists of the United States Geological Survey, whose reports have just been published as an advance chapter of the survey's Bulletin 430. This chapter includes two reports, one on deposits

in southeastern Idaho and adjacent parts of Wyoming and Utah, by H. S. Gale and R. W. Richards, the other on deposits near Ogden, Utah, by Eliot Blackwelder. The reports discuss the geologic age and relations of the deposits, their origin and the chemical composition of the rock and are illustrated by maps and geologic sections. The deposits are described and mapped in detail and estimates are given of the available phosphate in the several areas considered. The phosphate rock is chiefly of oolitic structure—that is, it consists of masses of round grains closely cemented together with other material, generally calcite. These grains differ greatly in size in each mass of rock, ranging from microscopic pellets to pebble-like bodies half an inch in diameter. Chips of shells and small fragments of plants are in places included in the rock. The rock at different places differs in color, ranging from gray to jet black, the darker shades being probably due to the presence of bituminous matter. The areas examined contain more than 267 million tons of high-grade phosphate rock, very little of which has yet been mined, and it is probable that the deposits extend far beyond the areas examined, forming, perhaps, the largest phosphate field in the world.

UNIVERSITY AND EDUCATIONAL NEWS

THE amount of the bequest made by Professor Goldwin Smith to Cornell University is \$832,000. The testator states that he makes the bequest "to show my affection for the university, at the foundation of which I had the honor of taking part, to pay respect to the memory of Ezra Cornell and to show my attachment as an Englishman to the union of the two branches of our race on this continent with each other and with their common mother." Professor Goldwin Smith bequeathed his library valued at nearly \$10,000 to the University of Toronto.

THE contract for an addition to the Ryerson Physical Laboratory at the University of Chicago has just been let, as the result of a gift by Mr. Martin A. Ryerson, president of the board of trustees and the donor of the

original building. The addition will be located back of the present laboratory, with which it will be connected by a Gothic corridor. The new building will be fitted with the newest and most improved equipment, and Professor Albert A. Michelson, head of the department of physics, and his staff, will then be in possession of greatly increased facilities for research. To this end research laboratories will be made a special feature of the new building.

THE basement of the geology wing of the science and museum building of the University of Colorado is completed. This wing is to be sixty by eighty feet in plan and three stories in height. It is being built of gray brick.

STATISTICS recently compiled at the University of Illinois show that there were 5,096 students in attendance at the university for the year 1909-10. These were distributed as follows:

Graduate School	283
Undergraduate colleges (not including professional)	3,491
College of Law	193
College of Medicine	526
College of Dentistry	108
School of Pharmacy	174
Academy	334

After deducting 13, those counted twice, we have the total stated above, 5,096. For the same year, 1909-10, the number on the instructional, scientific and administrative forces was 673. Of these 498 were in the schools and colleges in Urbana; the remaining 175 were in the professional schools of Chicago.

DR. WOODROW WILSON, having been nominated by the democratic state convention for governor of New Jersey, will offer his resignation as president of Princeton University at the next meeting of the board of trustees.

NEW appointments at the Oregon Agricultural College include Dr. E. G. Peterson, of Cornell, professor of bacteriology; Dr. J. F. Morel, in charge of the new work in veterinary science; G. R. Samson, U. S. Department of Agriculture, instructor in animal hus-

bandry; H. S. Marks, Cornell, instructor in mechanical engineering; J. F. Meister, Cornell, instructor in electrical engineering; G. F. Sykes, Brown, instructor in zoology and physiology; S. M. Dolan, Notre Dame, instructor in civil engineering, and Grace Campbell, Iowa State College, instructor in mathematics.

DR. MARTIN H. FISCHER has been appointed professor of physiology in the medical department of the University of Cincinnati.

ROY GRAHAM HOSKINS, Ph.D., formerly teaching fellow in physiology at Harvard Medical School, has been appointed professor of physiology in the Starling Ohio Medical College. He will be assisted by Dr. Clayton C. McPeek.

DR. A. J. GOLDFARB (C. C. N. Y., 1900, Ph.D. Columbia, 1910) has been appointed a tutor in biology in the College of the City of New York.

F. M. HANDY, M.A., has been appointed instructor in geology in the University of Colorado to take the place of Assistant Professor Ralph D. Crawford, who has been granted a year's leave of absence.

PROFESSOR GUY WEST WILSON, of Upper Iowa University, has accepted the position of assistant in vegetable pathology in the North Carolina Agricultural Experiment Station.

GEORGE D. HUBBARD, Ph.D. (Cornell), for the past five years assistant professor of geology at the Ohio State University, has resigned to accept the professorship at Oberlin College made vacant by the resignation of Dr. E. B. Branson.

DISCUSSION AND CORRESPONDENCE

THE SPECTRUM OF MARS

TO THE EDITOR OF SCIENCE: In the article by Messrs. Campbell and Albrecht, published in your issue of June 24, and read before the National Academy of Sciences at its April meeting, one is led to infer, though it is not expressly so stated, that the application of the Doppler-Fizeau principle to the study of the Martian atmosphere originated with Dr. Campbell.

Would it not have been more courteous to have mentioned the previous work by the same method by Dr. Slipher, along lines suggested by Dr. Lowell, and published in Bulletin No. 17 of the Lowell Observatory?

There is, moreover, such a striking similarity in the reasoning in the two articles, as to suggest that, though Dr. Campbell omitted to mention the bulletin, he had not neglected to read it.

G. R. AGASSIZ

TO THE EDITOR OF SCIENCE: The last paragraph of Mr. Agassiz's note suggests a charge, but thinly veiled, which no responsible man should make, certainly until after using all reasonable means for obtaining the other man's point of view. A basis for such a charge is to me unthinkable; overlooking the moral question involved, and commenting upon only a minor aspect, it is always the writer failing to give credit who suffers the consequence.

I am indebted to Mr. Agassiz's manuscript, which the editor has kindly forwarded to me, for my first information concerning an article on this subject by Professor Lowell. Looking up the reference, I find that Professor Lowell's article is stamped as received at the Lick Observatory on August 22, 1905. I was then in Spain observing the eclipse of August 30, 1905, and did not return to Mount Hamilton until November 22, 1905. I did not then, nor later, see Professor Lowell's article. None of my colleagues called my attention to it, and my first knowledge of it came to-day. The article was undoubtedly overlooked by and unknown to my colleague, Albrecht, also, or he certainly would have mentioned the subject when we were observing the spectrum of Mars, and especially when we were preparing our paper on the subject. I regret the oversight.

Professor Lowell's and Dr. Slipher's articles referred to form a four-page Bulletin of the Lowell Observatory. We have received neither index nor table of contents to the Lowell Bulletins, and probably none exists. The articles in question appear not to have been published

in any astronomical journal, either in full or in abstract, by Messrs. Lowell and Slipher; nor have I seen reviews of these articles by others.

When I was photographing the spectrum of Mars in December, 1896, with the high dispersion of a Rowland grating, fourth order, 14,438 lines per inch, as described in the *Astrophysical Journal*, volume 5, page 236, 1897, I realized that the Doppler-Fizeau principle offers great advantages for solving certain questions of the Martian atmosphere, as the water vapor and oxygen lines introduced in the Martian spectrum by the earth's atmosphere would be displaced with reference to corresponding lines in the Martian spectrum; but that the method could not be applied, with high dispersion, as the critical lines are all situated in the red, orange and lower yellow, for which regions sensitive plates were not then available. The isochromatic plates of that date gave under-exposed images. However, the question of applying the method by means of the three-prism spectrograph, which had then been in successful use for nearly a year, was thoroughly investigated to determine whether the dispersion of the three-prism instrument, when adjusted for the orange region, would be sufficient to separate or broaden appreciably the Martian and telluric lines when Mars was near quadrature in the first half of 1897. It was found that the dispersion was too low to afford any hope of success, and as the comparatively insensitive dry plates would not admit of higher dispersion, the subject was temporarily dismissed.

I find that Dr. Slipher's observations were first attempted in 1902-03, not published till August, 1905, and again early in 1905; but as his telescope had a smaller light collecting power and his spectrograph apparently a lower dispersion than I had considered using in 1897, his efforts failed. Here is his conclusion: "Measures were made, but they were difficult, uncertain and discordant, and neither proved nor disproved the displacement."

I note that while Professor Lowell recognized the existence of the method of solution

in October, 1902, as stated in his bulletin, he appears to have published nothing until August, 1905.

Appropriate notes will be published in the Lick Observatory Bulletins calling attention to Professor Lowell's and Dr. Slipher's articles, as well as to Dr. Slipher's results in low dispersion photography of the Martian and lunar spectra obtained in the summer of 1905, which led him to the conclusion: "No bands or lines could be seen in Mars that were not in the moon, nor any that were certainly stronger in the planet than in the moon. In short, the spectrum of Mars appeared the same as that of our equally high moon, so far as selective absorption is concerned."

W. W. CAMPBELL

Mt. HAMILTON,
August 15, 1910

QUOTATIONS

THE SHEFFIELD MEETING OF THE BRITISH ASSOCIATION

HUXLEY, in one of the last of his addresses, expressed some apprehension lest science should be crushed by the weight of the very gifts which she had demanded with such insistence from nature. The same thought has been present to many minds during the Sheffield meeting of the British Association, although it may not have been formulated with any precision. There was a time, not so many years ago, when men of science could aspire to the possession of an all-round acquaintance with many, if not all, departments of natural history, as it was then called. That time has gone by, and the infinite specialization which is a leading characteristic of science to-day is becoming more and more embarrassing to those engaged in the advancement of knowledge. This may be one of the causes of the comparative paucity of the numbers attending the Sheffield meeting. At first it seemed as if the members and associates would fall short of the number which took part in the previous meeting at Sheffield thirty-one years ago. Happily, this has not proved to be the case. There have been 1,449 members and associates this year, as compared

with 1,404 in 1879. Still, the fact remains that the number is the smallest recorded since the meeting at Dover in 1899. And this, notwithstanding the fact that Sheffield has doubled its population in the last thirty years, and increased enormously also in wealth and importance.

Many reasons are given for this state of things. By some it is attributed to the large number of congresses annually held in various parts of the United Kingdom and the Continent; by others to the lack of interest taken by the general public in scientific progress; by others, again, to the highly abstruse and recondite nature of many of the papers submitted to the sections. It is pointed out, moreover, that the number of scientific societies and institutions has enormously multiplied during the last few years, and that in these bodies there is a steady and frequent supply of reports and papers similar in kind and quality to those which it has been customary for so many years to contribute once a year to the British Association.

One thing is certain: that the president and council of the association are alive to the situation. They have given and are giving earnest consideration to the question of how to maintain in a high state of efficiency an institution which has played so honorable a part in the advancement of science in the past; and are resolved to put forth every effort to maintain its prestige and add to its usefulness. It is recognized that there has been too great a tendency in recent years to the creation of what may be described as water-tight compartments. In some of the sections, moreover, the papers read have been of so technical a character as to preclude all possibility of comprehension of them by more than a small number of highly-trained experts. The British Association exists to welcome to its meetings the results of the latest and most advanced research, but there is every desire to minimize the disadvantages attendant on specialization. Hence the large number of joint sittings of sections, which has been a notable feature of the Sheffield meeting.—*The London Times.*

SCIENTIFIC BOOKS

The Vegetable Proteins. By THOMAS B. OSBORNE, Ph.D., Research Chemist in the Connecticut Agricultural Experiment Station, New Haven, and Research Associate of the Carnegie Institution of Washington, D. C. Pp. xiii + 125. New York, Longmans, Green, and Co. 1909.

It would be difficult to name a scientist better qualified to review the present status of our knowledge of the vegetable proteins than the author of this monograph. For twenty years Dr. Osborne has unremittingly devoted his energies to the investigation of the problems in this domain; and any adequate presentation of the chemistry of plant proteins must consist in large measure of a résumé of his own contributions to the subject. Out of the chaos of the earlier work there has been evolved a more systematic knowledge of a group of compounds whose importance is just beginning to win appreciation and application in many departments of biological chemistry. Barely receiving mention in the treatises of yesterday, the vegetable proteins are to-day obtainable in a degree of purity scarcely approached in the case of the comparable compounds of animal origin. They are therefore supplanting the latter as materials for the study of protein structure and metabolism; and the development of protein chemistry is likely to receive greater impetus in the immediate future in connection with the products isolated from plant sources.

The present monograph has been written with characteristic accuracy and betrays first-hand knowledge of both facts and literature on every page. Here one finds the first adequate historical review of the subject, beginning with Beccari's experiments with wheat flour (1747) and the early story of gluten. A brief description of the occurrence of proteins in the different parts of plants is accompanied by chapters on the following topics: basic and acid properties of proteins; their solubility, precipitation, denaturing, and physical constants; products of hydrolysis; classification;

and some physiological relations. In the last chapter are included such timely topics as toxalbumins, anaphylaxis, haemagglutinins, and precipitin reactions.

Aside from numerous statistical tables of great value for reference, commendatory mention must be made of the unique bibliography of six hundred titles, itself one of the most useful, as well as the most complete, recent contributions to protein literature. No attempt has been made in the text to give working directions for students or investigators, or to furnish a descriptive account of the proteins. It is rather their properties, phenomena and relationships which are the subject of discussion. As an illustration of the helpful innovations introduced, the description of the acid and basic properties of proteins, and the relation of solubility to the free state or salt formation of proteins may be cited. The presentation is original and suggestive, in contrast with some of the current confusion of ideas on the subject.

The book is one of the series of Monographs on Biochemistry edited by R. H. Aders Plimper and F. G. Hopkins.

LAFAYETTE B. MENDEL

SHEFFIELD SCIENTIFIC SCHOOL,
YALE UNIVERSITY

Our Search for a Wilderness, an account of two ornithological expeditions to Venezuela and British Guiana. By MARY BLAIR BEEBE and C. WILLIAM BEEBE. Pp. xix + 387; appendices A, B and C. New York, Henry Holt and Co.

In "our Search for a Wilderness," Mrs. and Mr. Beebe have amply fulfilled the promise of their earlier book, "Two Bird-lovers in Mexico," and the present volume gives a delightful account of two journeys to northern South America. While the scientific results of these trips (and the collections made in their course) have been fully reported on by the New York Zoological Society, this narrative of the field experiences teems with interesting details of tropical life, and is written with evident enthusiasm and much charm. One closes the book with reluctance, and it can hardly fail to interest the casual

reader, while to the student of nature, in whatever degree of advancement, every page carries some suggestion or graphically describes some picturesque circumstance. The authors went through their journeys with their senses all on the alert, and the vivid sensations of the humid Tropics are as real as mere words can paint them.

It is to the bird-lover, however, that the book must make its strongest appeal, and every effort has been made to render the necessarily random notes and observations as useful and as accessible as possible, by devoting an appendix to the species of birds observed, and indexing each species, in the text, with a corresponding number. By this reference it is always possible to tell at once what species is under discussion.

Many exceedingly interesting observations, paying high tribute to the open-mindedness and keen sensations of the observers, relate to the protectiveness, in actual use, of many apparently bold and conspicuous color-schemes. The "Owl Butterfly," so long used as an example of "warning colors," comes into his own, and is shown, photographically, to be a marvelous composite of its rough-barked sanctuary on the tree-trunk, the "owl's eye" proving to be, instead of a conspicuous warning eye, a beautifully painted hole in the bark.

In the appendix giving the local native names of birds, it is interesting to notice the old habit of calling new birds by old home names, on the slender thread of fancied resemblance, here, as in other English-speaking outposts. Thus the familiar name of the European red-breast, "robin redbreast," is given in North America to a large thrush, in Jamaica to a tiny crimson-throated kingfisher (*Todus viridis*), and in British Guiana to a ground-starling! In this same appendix are noted vernacular names of birds not given in the list of species observed, and we are left in the dark as to the identity of such interesting-sounding species as "four-winged cuckoo" and "speculum parrakeet."

One of the best chapters is the one relating to "A Gold Mine in the Wilderness" although, in the narrative, the pay-streak seems

greater to the naturalist than to the argonaut. The charms of discovery here seem endless and entralling, and it is hard to call to mind a passage more replete with pioneer enthusiasm than this one. But each succeeding chapter carries new charm, and it is perhaps unfair to select any one as distinguished by its interest from the others. The river journey from the mines to the coast by canoe is as delightful a piece of descriptive writing as it has been our fortune to read. A real contribution, too, is the chapter on "The Life of the Abary Savannas," which contains a large amount of fine and original observation on the Hoatzin, an anomalous bird with reptilian tendencies and no close avian relationships.

The book is illustrated with well selected photographic half-tones, mostly by Mr. Beebe, and closes with a very complete and usable index. For the casual reader, as well as for the naturalist, it is replete with interest, and in places the excitement of scientific research, so generally quashed or altogether lacking, carries the reader into a new sympathy with the longing which leads men and women into the strange places of the earth.

LOUIS AGASSIZ FUERTES

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Experimental Medicine for September contains the following articles: "Effect of Various Agents on the Blood Flow through the Coronary Arteries and Veins," by G. S. Bond; "Another Point of Resemblance Between Anaphylactic Intoxication and Poisoning with Witte's Pepton," by Arthur D. Hirschfelder; "Studies on Immunity in Cancers of the White Rat," by Isaac Levin; "The Relation of Fatty Degeneration to the Oxidation of Purines by Liver Cells," by H. Gideon Wells; "Experimental Yaws in the Monkey and Rabbit," by Henry J. Nichols; "Changes in the Hemosiderin Content of the Rabbit's Liver during Autolysis," by W. H. Brown; "The Effect of Vagus Section upon Anaphylaxis in Guinea Pigs," by John Auer; "The Cultivation of the Leprosy Bacillus and the Experimental Production of Leprosy in the Japanese Dancing Mouse," by Charles W.

Duval; "Intracellular Proteolytic Enzymes of Liver," by A. R. Dochez; "The Cell Changes in Amaurotic Family Idiocy," by B. Sachs and I. Strauss; "A Transmissible Avian Neoplasm. (Sarcoma of the Common Fowl)," by Peyton Rous.

SPECIAL ARTICLES

THE PREVENTION OF THE TOXIC ACTION OF VARIOUS AGENCIES UPON THE FERTILIZED EGG THROUGH THE SUPPRESSION OF OXIDATION IN THE CELL

In former papers I had shown that the toxic effects of certain solutions on the fertilized eggs of the Californian sea urchin could be prevented by suppressing the oxidations in the eggs; either by depriving them of oxygen or by adding KCN to the solution. The solutions for which this was proved were: (1) hypertonic solutions, (2) hyperalkaline solutions and (3) solutions of certain neutral salts like LiCl, NaCl, KCl and others. The same observation as far as NaCl is concerned was made previously by O. Warburg.

I have continued these experiments this summer on the eggs of *Arbacia* in Woods Hole and find that the facts mentioned above are only special cases of a more general law. It is possible to prevent or diminish the toxic effects of the following agencies through the prevention of oxidation.

1. Neutral and alkaline salt solutions (with the exception of the salts of heavy metals).
2. Solutions of grape sugar (and probably other non-conductors).
3. Hypotonic solutions (*e. g.*, sea water diluted with equal parts of distilled water or a $\frac{3}{4} m$ solution of ethylalcohol).
4. Narcotics (chloral hydrate, phenylurethane, chloroform and alcohol dissolved in sea water).

In former papers I had shown that without oxygen no development of the egg is possible and it remained doubtful whether the life-saving effect of lack of oxygen under the conditions mentioned above was due merely to the inhibition of the morphological phenomena of development in the egg or to an inhibition of

the chemical reactions, especially oxidations underlying this development. The fact that chloral hydrate inhibits the development of the egg and that nevertheless the toxic effects of this substance upon the egg are inhibited by lack of oxygen or by NaCN indicate that the life-saving action of lack of oxygen in this case is due to the inhibition of chemical processes in the egg.

In former papers I had shown that the unfertilized egg is much more resistant to toxic media than the fertilized egg and I pointed out that this difference might be due to the difference in the rate of oxidation in both types of eggs. O. Warburg found that through fertilization the rate of oxidation is increased six times its original amount in the egg of *Strongylocentrotus*; and Wasteneys and I found that the consumption of oxygen rises in the egg of *Arbacia* to from three to four times its original value through the act of fertilization. We found, moreover, that the amount of NaCN necessary to prevent the development of the egg of *Arbacia* and to prevent the toxic action of the agencies mentioned above reduced the consumption of oxygen in the fertilized egg to from one third to one half the normal rate. The greater tolerance of the unfertilized egg towards these toxic media can therefore be explained by the low rate of oxidation in the egg.

In former papers, and especially in a book published a year ago, I pointed out that the process of membrane formation or a certain alteration of the surface of the egg is the essential cause for the starting of the development of the egg; and I pointed out, also, that this alteration of the surface might increase the permeability of the egg, especially for hydroxylions. It is indeed easy to show that in certain hyperalkaline solutions the fertilized egg of *Arbacia* gives off its pigment much more readily than does the unfertilized egg. R. Lillie, Harvey, McClendon and Lyon have recently published observations which in their opinion prove that the process of membrane

formation increases the permeability of the egg. I have found that a mixture of LiCl, KCl and CaCl₂ kills the fertilized egg of *Arbacia* even in the absence of oxygen more rapidly than the unfertilized egg, and it is possible that this difference in susceptibility between the unfertilized egg and the fertilized egg in the absence of oxygen is due to the fact that salts (or that part of the salts which undergoes hydrolytic dissociation) diffuse more rapidly into the fertilized than into the unfertilized egg.

JACQUES LOEB

ROCKEFELLER INSTITUTE,
NEW YORK

OSCILLATIONS IN ELECTRIC DISCHARGE

IN two papers recently published by the Academy of Science of St. Louis¹ the writer has called attention to oscillations in the air column of a Geissler tube, in connection with the stria. It was shown that the air particles are moving away from the negative terminal in open-air discharge. The Faraday dark space is a convection region. The air particles are supercharged in the region of negative glow, and then the discharge continues by convection across the dark space. The Crookes dark space in a vacuum tube is apparently a region of convection of the corpuscles themselves, before they reach the carriers.

The positive column is a drainage column where the negative discharge is by a conduction transfer from molecule to molecule towards the exhaust terminal. In the positive column, the air molecules are moving in a direction opposite to the drainage flow of the negative discharge.

The critical spark length is the length of the Faraday dark space. Dark convection discharge columns and luminous conduction columns then exist side by side. Electrically they are friendly, but mechanically they jostle each other about, by reason of the fact that the carriers in these columns are moving in opposite directions.

The proofs of these conclusions, which appear conclusive, are furnished in the photo-

¹ Trans., XIX., Nos. 1 and 4.

graphic plates reproduced in the papers referred to. It may be further pointed out that in minute spark gaps, such as are found useful in X-ray circuits, there is an oscillation which appears to promote the discharge. There is strong evidence which is being further examined, that these oscillations are due to an alternation of conduction and convection discharge across the gap. This involves a surging to and fro of the carriers, from one terminal to the other.

Faraday observed that there was no appreciable "electric wind" when a brush discharge becomes somewhat disruptive in character. He appears to have left Franklin's suggestion of a one-fluid theory wholly out of consideration. The mingling of convection and conduction discharges is sufficient to account for the phenomenon which Faraday observed. The brush discharge between two terminals becomes disruptive when the negative terminal is moved into contact with the end of the positive column. If the gap is made still shorter, until the distance between the knobs is equal to the length of the Faraday dark space, the critical spark length has been reached.

In the papers referred to, it was suggested that the stria in the Geissler tube were in the nature of vibrations in an organ pipe. This explanation simply involves the assumption that a wave consists of a dark space and an adjoining luminous segment. These are respectively regions of convection and conduction. They are Faraday dark spaces and positive columns. In them the carriers are moving in opposite directions. The nodal planes where pressure is at a maximum and at a minimum alternately both in space and in time, lie between the dark and luminous segments of the waves. At the instant when the pressure is at a maximum and a minimum at adjoining nodes, the convection transfer is zero. The conduction transfer will at that instant be at a maximum and a minimum at consecutive nodes. These conditions may explain the displacement of the striations which have long ago been observed.

FRANCIS E. NIPHER

THE INTERNATIONAL GEOLOGICAL CONGRESS AT STOCKHOLM

THE success of any great gathering of geologists may fairly be gauged by the men brought together, by the interest of the problems illustrated on the excursions, by the comfort and the pleasures of the entertainment, and lastly, it would seem, by the papers, conferences and discussions. Measured by all of these standards, the eleventh International Geological Congress, which was held in Stockholm during the month of September, will take a high rank among international scientific gatherings. The number of geologists in attendance was in excess of seven hundred, and the distinction of the names represented was noteworthy. From Germany came such men as Beck, Bergeat, Beyschlag, Credner, Groth, Keilback, v. Koenen, Penck, Rothpletz, Rudolph, Salomon, Sapper, Steinmann, Wahnschaffe and Walther; from Austria-Hungary, Brückner, Diener, v. Cholnoky and Tietze; from Canada, Adams, Coleman and Miller; from Denmark, Stunstrup and Ussing; from Egypt, Hume; from France, Barrois, Prince Roland Bonaparte, Haug, Kilian, Lory, de Margerie and Termier; from Great Britain, Cole, Garwood, Gregory, Horne, Oldham, Peach, Sollas, Strahan and Teall; from Italy, Baldacci, Capellini de Stefani and Mattiolo; from Japan, Inouye; from Mexico, Aguilera and Ordoñez; from Norway, Brögger, Reusch and Vogt; from Russia, Andrusow, Loewinson-Lessing and Tschernyschew; from Finland, Frosterns, Ramsay and Sederholm; from Sweden, Gunnar Anderssen, J. G. Anderssen, Bäckström, de Geer, Hamberg, Sven Hedin, Högbom, Holmquist, Lindbahm, Moberg, Nathorst, Nordenskiöld and Sernander; from Switzerland, Baltzer, Brunhes, Heim, Lugeon and Schmidt (Carl). The roll from the United States included Bascom (Miss), Becker, Bryant, Cross, Day (A. L.), Emmons, Fenneman, Grabau, Hague, Hobbs, Irving, Kemp, Lindgren, Newland, Reid, Richards, Smith (G. O.), Spencer (J. W.), Tarr, Van Hise, Winchell (H. V.) and Wolff.

The Swedish people enjoy a wide interna-

tional reputation for organization, and this could hardly be better exemplified than by the plans and their execution for the Stockholm congress, in which the general secretary, Professor J. G. Anderssen, has necessarily played the major rôle. The excursions before, during and after the congress were on a large scale, were participated in by an exceptionally large number of persons, and had an interest quite extraordinary. As many of them carried the visiting geologists well within the arctic circle, there was offered the opportunity of studying geological processes peculiar in some sense to sub-polar regions and hence largely new. Of especial importance and interest was the process of salifluction, apparently as characteristic of sub-polar latitudes as stream erosion of more temperate climes. The many quite remarkable manifestations of this process were to be seen by the members taking part in at least two of the excursions. The longest of the excursions, that to Spitzbergen, required over three weeks' time and was participated in by between sixty-five and seventy members of the congress. A grounding of the steamer in the Isfjord might have proved serious, but after a part of the coal had been thrown overboard the ship was again floated at the next high tide. About Torneträsk in northern Sweden, several of the other excursion parties were more or less united. Those interested especially in tectonic questions studied the great overthrusts of the district under the leadership of Professor Holmquist. The positions of the unmoved pre-Cambrian and Silurian beds and the great nappes of overriding crystallines could be discerned for long distances and with unusual clearness from the railroad, which follows the southern shore of the lake. Those more interested in glacial problems studied the successive ice-dammed lakes of late Pleistocene times which were formed against the ice front as it retired southeastwardly through this portal of the mountains. The strand-lines dating from this period are preserved in greater numbers and perfection than are even the famous "parallel roads" of the Scottish glens. The second of

the successive outlets toward the Atlantic is the Bardo Valley, and this was seen from a commanding position which had received no name. This magnificent gorge, while on a smaller scale than that of the Yellowstone, yet is its rival in many respects. The not distant railway station of Abiskojokk bids fair to become in the future a gathering point especially for European tourists, and the geologists of excursion A, unanimously voted that the beautiful point of inspiration from which the gorge was viewed be named *Point Sjögren*, in honor of Dr. Otto Sjögren, the leader of the excursion. Several of the large excursions included a visit to the great iron ores of the Kirunavaare district in northern Lapland, and many availed themselves of the opportunity to return to Stockholm by way of the northwest coast of Norway, with its many morphological and scenic attractions. In the Jämtland excursion, Dr. Högbom, the leader, was so unfortunate as to suffer a fracture of the arm, which compelled him to give over the conduct of the party, but did not prevent his attending the congress and taking charge of other excursions. One other accident marred the pleasure of the excursions. Professor Sapper, of Strassburg, was run down by a cyclist at the station of Are and his left arm broken and dislocated at the shoulder.

The complete list of the excursions is as formidable as the Livret-guide to explain them. This latter comprised no less than forty monographs, for the most part written with admirable adaptability to the end in view. These concise summaries of the geology of representative districts by the best authorities upon them, are being more and more highly appreciated by geologists, as is shown by the number who subscribe to the congress without attending its sessions. In addition to the Livret-guide a vast amount of literature was presented to members, and as most of it was valuable the manner of its transportation to the distant homes became sometimes an important problem for the visiting geologists.

Not least important of the acts of the Swedish committee of the congress has been

the discovery and use of a new method of conducting international investigations. As is well known, the method of appointing international commissions has, with one or two notable exceptions, proved ineffective. Such commissions were made to include the authorities in many countries, and for obvious reasons such a body proved too unwieldy for effective work. When it was possible to bring them together, discussion took the place of legislation, and authority to proceed along any definite line was wanting. In the Swedish plan the unwieldy commission is replaced by the council of the congress, whose responsibility is evident and whose interest to make the undertaking successful is immediate and acute. Having responsibility, they may delegate to any man or selected body of men both the general plan and the working out of the details of the inquiry. The permanent results of the inquiry take the form of a published report consisting of individual and generally brief summary reports from specialists in many countries, written in any of the four recognized languages of present-day science, the entire report introduced by a general summary of the reports written by a recognized authority who is essentially the leader in the inquiry. The volumes are edited by the general secretary of the congress.

The initial products of this system of international inquiry into geological problems are two reports of great value, one dealing with the iron-ore resources of the world and comprising two quarto volumes and an atlas, the other a large volume devoted to the changes in climate since the maximum of the last ice-period. It is proposed to employ the same method in an inquiry concerning the fracture systems of the earth's crust—the systems of joints particularly—as regards their orientation and interrelations.

The Swedish geologists had determined that the subjects especially discussed in the sessions should be those of most importance to their own country and also (in part) illustrated by the excursions. Two of these have already been discussed as the titles of the works issued at the opening of the congress,

viz., the iron-ore resources of the world and post-glacial climatic changes. Three additional subjects were chosen: (1) the geology of the pre-Cambrian formations with special reference to principles of classification and to deep-seated metamorphism; (2) the sudden appearance of the Cambrian fauna, and (3) the geology of the polar regions.

In addition to the formal ceremonies of the opening general session, there were read two papers which had a bearing upon the two most important topics of the congress. Baron de Geer with the aid of lantern slides sketched the outlines of his "geochronology of the last 12,000 years" based upon the study of the banded clay deposits in relation to the minor moraines of Sweden which are traced on either side of the Stockholm Os. The individual colored bands in the clay deposit, the *hvarfvig lera*, are believed to represent each the deposit of a single year within the submarine mouth of a former subglacial river of which the Os marks the course. Likewise there is a series of low but well-formed black moraines a few meters only in height and about 250 meters apart, which correspond each to a definite clay layer present on the south but lacking on the north and thus representing the delta deposit of the subglacial river for that year when the seasonal halt of the ice-front in its retreat laid down the moraine. With the aid of a number of students during many years, de Geer has worked out the entire series and thus derived the chronology. Two large excursion parties were taken into the field during the session of the congress, and some attempt was made to check the change in the number of clay layers at successive excavations separated by moraines. While this could not be wholly satisfactory in the time available, the methods of this novel and important investigation were learned and a corresponding profound respect was acquired for the ingenuity of plan and thoroughness of execution of the whole investigation—an investigation unique of its kind and one which will probably be followed by others in distant regions.

The other general address was by Professor Van Hise, on "The Influence of Applied Geol-

ogy and the Mining Industry upon the Economic Development of the World." This address sketched in broad lines the evolution of mine exploitation and in forceful manner presented the doctrine of conservation as applied to mineral resources, but especially to iron and coal.

The conferences and sectional meetings were so separated by excursions and by social or other events as to exert but little strain upon the members in attendance. The effect was apparent in the general good temper of the participants. The congress was formally opened on Thursday (August 18). On Friday morning those interested especially in pre-Cambrian geology joined an excursion to study the Archean of the vicinity of Stockholm, and in the afternoon they assembled in several sections for the reading of papers. The glacial geologists, on the other hand, devoted the morning to papers on glacial erosion, and in the afternoon studied the "hvarfvig lera" of the neighborhood under the leadership of de Geer. Saturday was given over entirely to papers, Sunday to an excursion to Upsala, Monday to papers, Tuesday to excursions of various kinds, Wednesday in turn to papers and Thursday, finally, to a general session on polar exploration and to the closing ceremonies. All notices except such as related to actions of the council were clearly and concisely given in a little book of convenient pocket size, which, if one had always with him, he needed to ask no questions in order to learn the time or place of any event. A list of the papers read by American and Canadian geologists in attendance follows:

F. D. Adams, "The Origin of the Deep-seated Metamorphism of the pre-Cambrian Crystalline Schists."

A. P. Coleman, "Metamorphism in the pre-Cambrian of Northern Ontario."

Wm. H. Hobbs, "Fracture Systems of the Earth's Crust."

H. F. Reid, "Faults and Earthquakes."

R. S. Tarr, "The Advance of Glaciers in Alaska as a Result of Earthquake Shaking."

W. G. Miller, "The Principles of Classification of the pre-Cambrian Rocks, and the

Extent to which it is Possible to Establish a Chronological Classification."

J. F. Kemp, "Archean Rocks of the Adirondack Area."

A. P. Coleman, "Methods of Classification of the Archean of Ontario."

A. W. Grabau, "Ueber die Einteilung des nord-amerikanischen Silurs." Also, "Continental Sediments in the North American Paleozoic."

F. D. Adams, "An Experimental Investigation into the Flow of Rocks."

A. L. Day, "Are Quantitative Physico-chemical Studies of Rocks Practicable?"

A. P. Coleman, "The Lower Huronian Ice Age."

H. G. Ferguson, "Mineral Resources of the Philippine Islands."

A number of important matters were settled by the council of the congress and announced at the closing session. The invitation of Canada to hold the next session in the dominion in 1913 was unanimously approved after the invitation of Belgium had been withdrawn. The council expressed its belief that the congress of 1916 should be held in Belgium. A proposition offered by Director G. Otis Smith for the preparation of a standard geological map of the world on the scale of one to one million was left in the hands of a committee consisting of Messrs. Beyschlag, Smith, Suess, Teall and Tschernyschew to present a plan at the next congress. The council recommended that the executive committee of the next congress take up an international investigation of the fracture systems of the earth's crust and publish a report in the manner so successfully prosecuted for the iron-ore resources and the post-glacial climatic changes. Professor Beyschlag reporting for the commission of the geological map of Europe announced that the sheets covering Central Europe are now out of print and that the commission has decided to issue a new edition of them. Messrs. Brock, Smith, Willis, Aguilera, Keidel and David were added to the commission.

Wm. HERBERT HOBBS

STOCKHOLM,

August 25, 1910